

Osteoarthritis and Cartilage



Letter to the Editor

Response to Letter to the Editor: 'Autologous chondrocyte implantation for the treatment of cartilage lesions; randomized control trials assessed in a systematic review'

We want to thank Drs Vasiliadis and Salanti for their letter. First of all it is most gratifying to see that another, independent group of investigators shared our notion that a systematic review of autologous chondrocyte implantation was needed and that they reached essentially the same conclusion despite using a methodologically different approach¹.

Vasiliadis and Salanti make two important points. First, the overall quality of the studies published on autologous chondrocyte implantation is rather poor, which implies a number of concerns about the validity of their findings. Also, the amount of information given on study design and methods in the included primary studies was somewhat limited, leading to slightly different interpretations between the two reviews¹. Moreover, assessing items of study quality is not as clear-cut as it is often assumed, as the letter by Vance Berger (see below) illustrates.

Secondly, there is substantial overlap between and within studies. The essential problem is that multiple published papers draw from the same population, but only some with new study questions. Vasiliadis and Salanti point out that we had claimed to include nine studies, but we like to direct their attention to the first paragraph of the results section where we tried to untangle the six trials vs nine papers situation. Knutsen *et al.*, for example, report largely the same clinical outcomes at two different time points² and 5 years, thus we considered this a "two papers out of one trial" situation. As Vasiliadis and Salanti points out, there was much dispute whether Horas *et al.* changed more than the language of publication between their two publications or actually presented a (ny) new study question(s) in their second paper². Saris *et al.*, in turn, had major change in endpoints when they reported histology at 18 months, but MRI at 36 months. Does such a change in the primary outcome qualify this as a new study building on the same population, or is this also a two papers out of only one trial situation?

What stands out from all this is, as Vasiliadis and Salanti point out in their letter, that there is a considerable need for high-quality studies with systematic, high-quality reporting in

the field of cartilage repair. However, despite the state of the "current best evidence" in cartilage repair, it is interesting to see that two independent systematic reviews^{1,3}, with each possessing distinctly different methods, arrived at consistent results suggesting a favorable outcome after ACL, after adjusting for potential bias.

Conflict of interest

None.

References

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